SCIENCE

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PHYSICAL SCIENCE IN THE SECONDARY SCHOOLS.

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IN view of the fundamental changes in methods of education within the last twenty-five years in testing experimentally the educational value of subjects which formerly were not recognized as a part of liberal training, it is to be expected that the secondary schools should await the results of such trials in the higher institutions. The time and energy devoted to the comparative efficiency of different methods or the comparative value of different subjects in these schools should be extremely limited. They can afford no loss of time in uncertain paths. It is more reasonable and economical to leave all teaching in experimental methods to the mature judgment and experience of educators who have devoted their lives to this subject, and who have at their command the ample resources of the college or the university, with no constraints in the employment of their best judgment, such as unavoidably exist in the common schools.

With little thought it might seem that the teaching in the common schools, with aims and methods widely divergent from those in the higher grades, can be critically studied only by those who have them in charge. Upon close examination, however, it is evident that this difference is only one of degree, and experience has shown clearly that the best suggestions for elementary training come from persons engaged in higher teaching, or from persons who are able to adapt methods of higher teaching to the wants of younger pupils. Indeed, the methods universally accepted as the most worthy are the results of study and investigation in the fields of advanced knowledge.

Nevertheless, with the best methods and all the wisdom and judgment of generations of experienced educators, any system of education may fail utterly if it is not supported by teachers who have an enthusiastic interest in imparting knowledge. Without the personality of such a teacher, one subject as well as another may fall into a tedious, uninteresting routine Even in laboratory training, in which it is not difficult to maintain a lively interest, the teaching may easily take a form which fails to accomplish the especial objects for which it is intended. The study of natural phenomena under skilful guidance results in the production of self-reliant students.

In the domain of natural and physical science, instruction may now be considered as having passed the experimental stage, not only in the higher grades, but in the common schools, and the educational value of such instruction is recognized as a part of liberal education. Aside from the practical information, which is a part of general knowledge, the characteristic benefits of scientific training appear in the thorough discipline in methodical habits of study and an intelligent use of the perceptions. If these are the results of the study of science in the higher grades, why may not the same methods, simplified and properly applied, form a part of the means for the development of younger pupils? When we consider the great breadth of the field of knowledge and the limited span of the average human life to compass it, it seems a very short intellectual step from the development in the mind of the child to the more mature condition of the youthful intellect as it passes through the various stages of collegiate training.

In most high schools attempts are now made to teach physics and chemistry, but under very adverse circumstances. There are certain difficulties to be overcome in the proper development of such teaching, but they should not be looked upon too seriously. The earlier condition, in which Latin and Greek were selected as soon as the student had covered the ground of the elementary English branches, is, happily, adjusting itself on a reasonable basis. Perhaps it is of more importance that the wide range of subjects included in the average high-school course must result in a slight and superficial knowledge of many things rather than a thorough training with reference to correct habits of thought and study in any direction. Probably the more serious hindrance to laboratory teaching in chemistry and physics to classes of any magnitude is the expense of the necessary appliances and a lack of knowledge of a proper and economical expenditure of time and energy.

The utility of physical science properly taught as a means of mental culture and discipline has been fully demonstrated in the rigorous tests it has withstood in the severe criticism of modern educational methods. The particular value of such teaching is manifest in the opportunities it affords for accurate observation, exercise in methods of inductive reasoning, and practice in recording the impressions in the form of notes. The most satisfactory as well as the most convenient method of imparting knowledge of the principles of physical science to classes is by lecture-table demonstration. Text-books may be used as an aid, but the personality of the instructor behind illustrative experiments is the most direct, and in fact the only method whereby an eager interest can be aroused in the pupil. Lecture-table demonstration and laboratory practice under the immediate oversight of an intelligent instructor should proceed hand in hand.

There is still another view, which, it seems to me, is worthy of consideration. Probably no one will deny that practical knowledge should be imparted whenever it is consistent with proper mental discipline. Certainly there are important reasons for including as much practical information as possible in any high-school course. Most of the young men who graduate engage in business, and a comparatively small number of the young women continue their studies beyond the high school. Any young man in business has a constant use for knowledge of the chemical composition of substances, their physical and chemical properties, and their uses. Much of such information may easily be included in elementary courses of instruction. Every young woman should understand the principles of ventilation, of sanitary appliances, the applications of weights and measures in the household, and the ordinary chemical changes which are the basis of the preparation of foods, as well as the influence of temperature upon such changes. The ordinary chemical changes in bread-making, in fermentation, in decay, and similar operations should be common knowledge; yet there are, doubtless, very few of the young lady graduates of the high schools who possess a correct knowledge of this subject.

I am well aware of the apparent difficulties in the way of developing laboratory instruction, and I shall venture to propose methods which may be readily applied in any high school. If it is granted that the results of suitable instruction in elementary physical science are worthy of the effort, these difficulties are limited to two directions, and they may be easily overcome. Perhaps the most serious obstacle is the expense of equipment and maintenance of laboratory practice. In a room 40′ by 30′ forty-eight desks may be arranged with ample accommodations for ninety-six students working in two divisions, or for one hundred and forty-four students working in three divisions, with separate drawers and lockers for the apparatus of each division, and with all necessary hood-space and sinks. The cost of the arrangement of such a laboratory, including all gas-fitting and plumbing, and all reagent bottles, in fact fully equipped, except with apparatus for individual students, should be less than $1,500. The cost of apparatus needed by each student should not exceed

$5. The cost per year of chemicals and of apparatus to provide for breakage should not exceed $6. A class of twenty-five students can be accommodated in a room 20' by 27', and the expense of a complete equipment should not exceed $700. For smaller classes the cost should be proportionally less. These estimates are based on the results of extended experience in the construction and arrangement of several laboratories, which it has been my fortune to superintend.

The second obstacle mentioned above has reference to the large teaching force which would apparently be required in such instruction, but it would seem that it is rather a result of a want of knowledge of the best methods of teaching physical science, both on the part of executive boards and of many teachers themselves. From the results of my own experience in similar grades, as well as in more advanced instruction, I am convinced that this difficulty is only apparent. In a high-school course of three or four years, physics should be taught during the junior year and chemistry in the senior year.

In chemistry, two hours a week should be devoted to lecture demonstration, with two afternoons, of two hours each, to laboratory work, and one hour to a recitation on the subjects of the lectures and laboratory practice. The same method should be adopted in the physics of the third year, although from the nature of this subject perhaps a text-book may be used more freely.

The same laboratory will serve for both physics and chemis ry, and in physics the same apparatus will serve for different students. Hitherto the chief difficulty in teaching experimental physics has been the high cost of the apparatus; but suggestions concerning inexpensive forms of apparatus have recently been given for the benefit of the secondary schools by professors of physics, especially by the professors at the Jefferson Physical Laboratory of Harvard University, and such apparatus is for sale by the dealers at a small cost. The same instructor may have charge of physics and chemistry, and the success of such teaching would depend upon his particular qualifications. He should be allowed at least eight hours a week to prepare for class-room and laboratory exercises, with some aid from the janitor or other servant. He should still have considerable time which could be devoted to such other teaching as might seem expedient, perhaps in some other branches of science. In the high schools outside of the larger cities the annual salary of an instructor should be between $600 and $1,500, depending upon the size of the school. One instructor can easily teach a class of thirty members; in the larger schools, laboratory assistants would be necessary; lady teachers with suitable preparation are very successful in laboratory teaching, and this service could be combined with other duties.

I am aware that excellent training in elementary physical science is given in some of the high schools in the larger cities; but, notably throughout the West, such teaching, when it is given at all, is usually confined to routine text-book methods, with little, if any, experimental illustration, at least by the students themselves. Such a system as the one herein described requires certain small expenditures, but the efficiency of the high-school instruction would thereby be greatly improved, and the public would soon appreciate the importance of sustaining efforts leading to broader and more practical training.[1]

If at first the governing boards of high schools should feel the need of suggestions in the preparation of plans and estimates for equipment of laboratory rooms, I am sure that professors in charge of laboratories would gladly render such assistance. The success of this system requires a knowledge of special methods, which many teachers do not possess, but they are enabled to acquire it in laboratories which are open during a part of the summer vacation. The chemical laboratory of Harvard University was first opened during the summer of 1873 for the benefit of teachers, and many now have charge of responsible teaching through the knowledge acquired by continuous attendance during successive vacations.

[1] Every citizen is directly interested in the welfare of the public schools, and all parents will heartily support any endeavor looking towards the attainment of the greatest amount of useful knowledge, as well as the best mental development for their children.

What has been said about physical science in the secondary schools may apply in a different sense and on a higher plain to the condition of scientific training in many colleges. The increasing demand for the admission of college graduates to advanced standing in schools of science should be encouraged, since the discipline of a collegiate course is an excellent foundation for advanced scientific study, provided it includes thorough instruction in the elementary branches of science. A college course should offer, as a part of its required work, comprehensive training in general and descriptive chemistry and descriptive physics with extensive laboratory practice in both subjects. Most colleges can also give elective instruction in qualitative chemical analysis, with some additional study in quantitative analysis Graduates from such courses, which should also include French and German, are well qualified to enter the junior year in the best scientific schools. Unfortunately, at present, not all colleges give a sufficiently thorough drill in elementary physical science, in consequence of which many graduates who desire to enter schools of science labor under a serious disadvantage from a want of the more elementary knowledge. Most colleges, doubtless, feel that they devote as much attention to scientific subjects as is consistent with the thorough general training that is expected in a college course. While this may be true in part, it must be admitted that thorough training in physical science should now have as important a place in a college course as mathematics or the ancient languages. It is not to be expected that the college can provide the expensive equipment for the study of science that is the foundation of the school of science. But every college can afford the small expenditure that will thoroughly equip and maintain working rooms for the use of elementary physics and chemistry, with sufficient instruction to render this study interesting and profitable. The feeling of mutual interest and dependence between the secondary schools and the scientific schools, and perhaps in a less degree between the college and the scientific school as a professional school, should be promoted and encouraged; and whatever aid it is possible to render in either direction should be cheerfully granted.

OUR VACANT PUBLIC LANDS.

BY F. H. NEWELL, WASHINGTON, D.C.

THE total area of the public lands vacant in 1892 has been estimated by the Commissioner of the General Land Office at, in round numbers, less that 568,000,000 acres, these being located in 25 states and territories. Of this total by far the greater part, as is generally known, is in the western half of the United States and mainly west of the 100th meridian. Taking therefore the Dakotas, Nebraska, Kansas and Texas, and the states and territories to the west of these, numbering in all 16, these contained nearly 542,000,000 acres, or about 95 per cent of the vacant public lands. The remaining 26,000,000 acres in the nine political divisions to the east of the states named may be considered as of little value, at least for homesteads. A great part of this is in the swamps of Florida and Louisiana or in what are generally considered non-agricultural regions of Arkansas, Michigan, Minnesota and Wisconsin. The very fact that these lands have not been taken up, although open to settlement for many years, testifies as to the doubts or failures of would-be settlers.

The rate at which the public lands are being sold is also should in the reports of the officer above mentioned, from which the following figures have been culled:

Disposal of Public Lands.

1890	12,798,837	acres
1891	10,477,700	"
1892	13,664,019	"

During the year 1892, the disposal of lands has been abnormal in quantity, owing doubtless to several causes, but mainly from the legislative or official side rather than from increase of settlement. As a rule it may be said that the sales of public lands have been steadily decreasing year by year until 1892, when

they suddenly rose far above the average. This is shown by the following brief statement of the original homestead entries:

Comparison of Original Homestead Entries.

1888..........	6,676,616 acres,	a decrease of 917,734
1889..........	6.029,230 "	" " " 647,386
1890..........	5,531,679 "	" " " 497,551
1891..........	5,040 394 "	" " " 491,285
1892..........	7,716,062 "	an increase of 2,675,668

Taking the average annual disposal of the public lands at 12,000,000 acres, and assuming all the vacant land susceptible re-entry, it would be entirely taken up in less than 50 years. As a matter of fact, however, only a small portion of this vast area can be acquired under the operations of the present laws or is suitable for homestead purposes. A great part consists of high mountains or deeply-eroded plateaus, of sterile lava-covered plains, or is too rough to be valuable for agricultural purposes. What may be considered as the choicest portions of this vacant public land, where the soil is deep and rich and can be readily tilled, are at present almost valueless on account of the aridity of the climate. While on the one hand mountains, canyons and lava plains cannot be removed, yet on the other the aridity, or at least its effects, can be modified to a certain extent, and lands with fertile soil now useless can be added to the producing farm areas of the country. This aggregate area, however, is relatively small, and at the present rate of disposal of public lands it is a question of only a few years when every available acre will be taken.

Under the operation of existing laws, the rate of disposal of vacant public lands must naturally be constantly diminishing, and it follows, that the probable time of disposal of the lands must be indefinitely prolonged. This decrease in sales or number of homestead entries is, of course, not due to diminution of the demand, for each year this is growing greater and greater, but is the result of scarcity of supply. As previously stated, the more available lands have been taken, and each year the choice is more limited, and men are compelled, by circumstances, to enter upon lands which a few years ago they would not have considered worth taking up. In this state of affairs public interest is being turned to questions bearing upon the reclaiming of portions of the remaining public lands, and greater eagerness is shown in developing all the resources by which these may become valuable.

The results of the eleventh census of the United States, as they have been published, cast light upon some points hitherto obscure, bringing out the condition of development of the western part of the United States, as well as of the whole country. Among other facts, the enumeration has shown that the area irrigated in 1889 was 3,631,381 acres. The scattered patches which go to make up this amount were located from points west of the 100th meridian to the Pacific coast, with the exception of the western part of Oregon and Washington. The total land surface of this area, deducting the 36 counties of western Oregon and Washington, is 1,380,175 square miles, or 883,312,000 acres. The area irrigated thus formed about four-tenths of one per cent of this vast country, which contains nearly all possible combinations of soil and climate, ranging from the smooth, almost arren plains, with scanty vegetation to the high, rough mountains, whose peaks are covered with snow throughout the year, and whose slopes have been clothed with thick forests.

Looking at this vast extent of arid and sub-humid land in a broad way, it is possible to distinguish four great classes, according to the amount of moisture received, or the water supply available, as shown by the character of the vegetation, viz., desert, pasture, fire-wood and timber lands. These may be defined as follows: The desert land is that within which the water supply is so scanty that cattle cannot obtain sufficient for drinking purposes, and the vegetation so ephemeral that it has little value for pasturage. The soil, however, is often rich, and when watered, produces large crops. These desert areas of the United States are, however, rarely without vegetation, and the large amount and variety of plant life are often matters of astonishment to the traveller.

The second class, the pasture land, may be said to embrase all of the Great Plain region which, on account of prevailing aridity, is useful mainly as pasturage. The localities at which agriculture is possible are relatively of insignificant size, although of great importance in a grazing country. It also includes the valley lands within the Rocky Mountain region and the rolling hills on which native grasses grow.

The fire-wood land may be defined as that fringing the timbered areas, and intermediate in character between the pasture land and the high, rough, forested slopes or plateaus. It includes also precipitous hillsides found at an elevation too low to receive a large or constant supply of the moisture which falls upon the more heavily timbered areas.

The fourth class embraces the forested areas upon the high mountains where the conditions are such that trees have been able to attain a size suitable for timber. With this understanding, the following table is given:

	Acres.
Desert land..........................	64,000,000
Pasture land..........................	620,912,000
Fire-wood land........................	115,200,000
Timber land.......................	83,200,000
Total.......................	883,312,000

Of this total, as above stated, less than 568,000,000 acres still belong to the general government.

The irrigated and irrigable lands are mainly included within those divisions which in their natural state have been considered as desert or pasture land. In a general way, it may be stated that fully nine-tenths of this area is covered with a fertile, arable soil which only lacks sufficient moisture in order to be of value for agriculture. If this proves to be the fact, then out of this total of, in round numbers, 616,000,000 acres of arable lands less than six-tenths of one per cent was irrigated in the census year. As to the reclaimability of a large portion of this area, the question of water supply obviously must first be discussed.

CONTRIBUTIONS FROM THE LABORATORY OF THE YORK COLLEGIATE INSTITUTE.

BY C. H. EHRENFELD, YORK, PA.

Effect of Burning on the Volume of Limestone,

In the York, Pa., courts recently, a case was tried which involved the question whether limestone shrinks by being burned. The matter was submitted to me to be tested. On consulting authorities I found the statement given that no shrinkage occurs; but no method was given for making the test. Hence I devised methods as follows: Several pieces of limestone of varying firmness of texture were taken, and permanent marks made upon them. The distance between these marks was accurately measured. The pieces were then burned in a gas furnace at a high heat for about seven hours. After cooling, the distances were again measured, and were found to be unaltered. The pieces were then slaked with water, to ascertain if the burning was complete. Another test was made in the following manner: The pieces of stone were dipped into melted paraffin and quickly removed in order to coat them with a very thin layer of paraffin, sufficient to render them impervious to water, but not enough to add materially to their volume. Their volumes were then determined accurately by lowering them into a graduated vessel partly filled with water. After being burned, the pieces of stone were again dipped into melted paraffin and the volume determined as before. It was found that no change whatever had taken place.

Water in the Spheroidal State.

While carrying on a piece of work recently which involved the use of a common Liebig condenser, it was noticed that where the stream of waste water fell into the water-trough, the bottom of which was rough, small globules of water were formed, which darted out on all sides and ran on the surface of the water to the sides of the trough, eight or ten inches distant. Frequently

they would rebound from the side and start back, but would soon disappear. The globules varied in size from an eighth of an inch in diameter to very minute. Sometimes while running along they would gradually decrease in size until they would disappear, while others would disappear in an instant. In a few cases the size suddenly decreased to about one-half the original diameter, the globule then continuing on its course without further change, until it at last suddenly disappeared. Sometimes two globules would run together, combine, and continue on their course as one globule of increased size. In other cases, instead of combining they would rebound from each other like rubber balls. This rebound also took place when they ran against an air bubble. In one case a globule about one-eighth of an inch in diameter reached the side of the trough and rebounded, but it was reduced in size to about one-half of its original diameter. It was noticed, also, that they did not all move with the same velocity: some shot across the water with great rapidity, while others moved very deliberately, both kinds of movement taking place at the same time and in the same direction. In rare instances the globules stopped and lay at rest on the surface of the water until their final sudden disappearance. The rapidity was always greatest at the beginning. In order to ascertain how rapid a current there might be (the water was about a quarter of an inch deep) bits of wood were floated on the surface. The current thus indicated was many times slower than the movement of the globules.

Particular attention was given to ascertain at what place the globules originated. The falling stream made a circular depression in the water about an inch in diameter. The globules seemed to spring up from the outer edge of this depression, fall back on the surface of the water, and then run rapidly away as described above. The thought suggested itself to me that many, if not all, of the observed phenomena could be accounted for by rapid whirling motion of the globule. The gradual slackening of the motion, the fact that some stopped on the surface of the water, the quick rebound from the sides of the trough, are all effects which can easily be produced by a rapidly whirling ball on a plain surface, like the well-known movements of a billiard ball. This would also account for the phenomenon of a ball of water floating on water, without blending with it, somewhat on the same principal that stones can be made to skip over the surface of water without sinking at once; or more remotely, as the pitching of a curve in base-ball. The conditions, too, at the place of origination of the globules, were just such as would produce a sharp twisting motion. The falling stream was first turned to the side by the bottom of the trough and then upward, until at the top of the rebound the little globules sprang out.

I do not recall ever having seen the above explanation given, and so it is offered for what it is worth.

The temperature of the water was never above 30°C., which would preclude the common explanation for high temperatures. Afterwards the same effects were obtained, on a smaller scale, when the prongs of a large vibrating diapason were dipped into water to show the effects of vibration.

THE HIEROGLYPHICS AND SYMBOLS OF ANCIENT MEXICO.

BY FRANCIS PARRY, F.R.G.S., LONDON, ENGLAND.

The inquiry into the construction of the hieratic writing of the Maya people, drags its extended length over many a passing decade, and does not go forward by leaps and bounds. So it has been with the investigation of the groundwork of the symbolism of the temples, the carved slabs of Palemké, the monoliths of Copan, the profusely ornate external walls of the numerous temples of the Yucatan peninsula. This symbolism is the very foundation of the whole matter, the essence of the spirit pervading the sacerdotal mysteries of Central America.

Mainly graven on stone, its variations are noticeable at a date far from and greatly preceding the manuscripts, consisting of the limited number of four, that have been transmitted to us. These written records, probably because of their being in a form affording an easier study than the numerous drawings representing the many sculptured remains of ancient Mexico, have had the attention of the book student fixed upon them in no ordinary degree. This concentration of thought has been a hindrance to progress, inasmuch as it surveyed a comparatively narrow field, and, observation not reaching far enough, the rise of the hieroglyphic forms, the initial composition of the hieratic writings, and the evolution of religious thought, giving life and spirit to the whole, has been but partially traced.

In order to obtain a firm grasp of the situation, the view should be extended, and broadened to the utmost bounds of our knowledge. Primitive rock scratchings, the roughest sculptured stones, the cup and ring incised carvings of prehistoric times, — each and every source of information should be called upon to contribute material.

In all study connected with hieroglyphics, in fact in all scientific research, an endeavor to find radicals, to establish simply foundation truths, and follow the processes of Nature or the compositions — the artistic productions — of the fertile brain of man from the lowest source, is the surest way of following the ramifications of evolution.

Persistent efforts to break up the mass of concrete Maya symbols have, during a century, given results that have been disappointing. Had the clue been discovered the entire outline of the sacerdotal system must have been traced. The United States Government has, however, largely contributed towards the attainment of a perfect knowledge of these ancient mysteries, by lavishly aiding inquiry and publishing from time to time records, the work of professors, accompanied by engravings which, as ideographic forms are a main feature of the system, are invaluable when the consummation of the inquiry is about to be reached.

To state that the end has been reached would be to assume the subject of Maya symbolism is exhausted. I may, however, confidently predict we are on the high road to the desired goal and announce the striking of a vein, the discovery of the lode, and invite scientists to scrutinize my observations upon that Maya relic, "The Sacred Stone." The whole question of its identity, is treated in a popular manner in a monograph entitled, "The Sacred Maya Stone of Mexico and its symbolism." The stone had been misnamed, and its use conjectured. Supposed to be connected with the ancient Aztec ritual or sacrifices, it was given an incorrect place chronologically, historically.

In the museums of the United States and throughout the archæological collections of Europe, it has been classed as sacrificial. That excellent serial, "Archives International d'Ethnographie," published in Leiden, has in Volume III. an exhaustive disquisition on the many varieties of the stone by Herr Strebel of Hamburg. The conclusion he arrived at is the rejection of the nomenclature of the museums. In this result I heartily concur, but taking an independent view and a new departure, I venture to assert and am prepared to prove it to be a relic of paramount interest. Its earliest archaic type is the key to opening out a vista of a nature worship of wide extent, and the ornate, highly finished examples demonstrate evolution, in religious thought, a recognition of combined natural forces, and *solve the mysteries.*

CURRENT NOTES ON ANTHROPOLOGY.—XXVI.

[Edited by D. G. Brinton, M.D., LL.D.]

The Ethnic Study of Religions.

A SUGGESTIVE sketch on "Recent movements in the historical study of religions in America" appears in a late number of *The Biblical World* from the pen of Professor Morris Jastrow, Jr. He details the progress of the historical and comparative study of religions, both in this country and in Europe, and very properly urges its importance as a branch of instruction in universities and similar institutions.

It appears, however, that it is now generally taught as a branch of psychology, ethics, speculative philosophy or doctrinal instruction. This is unfortunate, as these are not the real and nearest relations to religions. Their closest ties are to ethnic characteristics, and only by the light of these can they be clearly

comprehended. This is nowhere better illustrated than in the religions of the two great branches of the White Race, the Semites and Aryans. As Dr. Heinrich Schurtz points out in his "Katechismus der Volkerkunde," Christianity, which is ethnologically a polytheism, has been and remains as distasteful to the Semite, as are his localized monotheisms to the Aryan. "The greatest triumph," remarks Mr. G. L. Gomme, in his excellent little book, "Ethnology in Folk-lore," "of the Aryan race was its emancipation from the principle of local worship." It is tied neither to Mecca nor Jerusalem.

These characteristics of religions which obtain historic permanence, find their roots in marked ethnic features, as the tendency to abstraction among the eastern Aryans; and the sphere of their influence is limited by these. Proselytes of another race do not accept the religion as it is taught them, because they cannot. They are proselytes in name only. As Karl von den Steinen remarks of the Christianized natives of Brazil, ' They understand its real doctrines about as much as they do the theory of spectral analysis." Only when the historical and comparative study of religions is prosecuted definitely as a branch of ethnology can it attain the best results.

The Stature of the Most Ancient Races.

Has the species of man increased or diminished in stature since it first appeared on this planet? Have his bones increased or diminished in solidity and weight? Have the relations in these respects between the two sexes always been as they are now?

These are some of the very interesting questions approached by Dr. J. Rahon in a recent paper in the Memoirs of the Anthropological Society of Paris, entitled, "Recherches sur les Ossements Humains Anciens et Préhistoriques." It occupies about sixty pages, and is the fruit of most laborious and creditable investigation, both in the collection and digestion of facts.

His conclusions may be briefly stated. Comparing the earliest quaternary skeletons found in western Europe with those of the present population, the former belonged to what we should call medium-sized people, with an average stature, of the males, of 1.63 metres. The tribes of neolithic times varied scarcely at all from this measurement; but the proto-historic nations, the Gauls, Franks, Burgundians, etc., ran the figures up to a mean of 1.66 for the males; since their epoch it has been steadily, though slowly, descending, at least in France, until the average of the Parisian men of to-day is 1.62 metres.

In all ages, the women have averaged about ten centimetres less in height than the men. The bones of both were rather heavier and more powerful in ancient times.

Incidentally, Dr. Rahon shows that the height of the men of Cro Magnon has been over-estimated; that of the man of Spy under estimated; that the Guanches of Teneriffe averaged but one centimetre above the French of to-day, and osteologically were very similar to the Cro Magnon people; that from the most remote time the human body has retained the same porportions; and other suggestive inferences.

The Character of the Glacial Epochs.

The "glacial period" has its greatest interest because it seems to have occurred about the time that man first appeared on earth. Two careful studies of it have recently appeared in *Das Globus*, one by Dr. von Ihering, in an article on the "Palæo-Geography," of South America; the other by Dr. Nehring, in reference to Europe.

In spite of some recent claims to the contrary (see *Science*, March 11, 1892, p. 146) Dr. von Ihering is positive that the birthplace of the human race need not be looked for in South America. Its chief land-mass was once connected with Australia and Africa; but this connection was broken in middle tertiary times. Sometime in the pliocene it first became connected by a land-bridge over Florida and Cuba with North America, and an extensive interchange of mammals took place. The Pampas are pliocene, and show no signs of glacial action. This appears in the pleistocene, and the great glaciers of South America were contemporaneous with those of North America.

Dr. Nehring has occupied himself with tracing the distribution of the steppe fauna into Central and Western Europe in quaternary times. His conclusion is that it extended widely in this direction at a certain period, which he believes marks an interglacial epoch, covering thousands of years, and characterized by a comparatively dry and mild climate, and a notable diminution in glacial activity. The displacement of the steppe fauna, which then flourished in Germany and France, by an Arctic fauna, points to the re-establishment of glacial conditions.

Geologists as well as naturalists are fully alive to the multiple bearings of glacial events on diverse branches of science. The new *Journal of Geology*, started this year by the University of Chicago, has its initial number principally made up of contributions on glacialism. One of them, by Mr. W. H. Holmes, on "Glacial Man in the Trenton Gravels," is distinctly archæological. He sets forth the difficulties in the way of accepting the evidence advanced, and, while rejecting it as inadequate, does so in a fair and unprejudiced tone.

Ethnography of Central America.

Among those whose published studies have considerably aided in the advancement of knowledge concerning the geography, archæology and ethnography of Central America, M. Désiré Pector, consul of Nicaragua at Paris, deserves an honorable position. He has been for years an active officer in the Société Américaine de France, and in the Congrès International des Américanistes. Among the various articles which he has recently issued, one touches on the origin of the name America. This has been derived by Marcou and others from the native word "Amerrique," applied to a chain of mountains on the Atlantic coast of Nicaragua. M. Pector, however, shows that the correct form is "Amerrisque," and rejects the Marcou hypothesis.

In a more extended study, M. Pector takes up a large number of the native geographical names of Central America, and attempts to trace their etymology. It is in part an appendix to an earlier essay on the localisation of the principal tribes of that region at the time of the conquest. Unfortunately, many of the Central American languages are so little known that their methods of compounding words are obscure, and such studies can at present be little more than gropings.

The archæology of Salvador affords him another theme, which he treated in the Archiv. Internat. d'Ethnographie last year, apropos of Montessus de Ballore's book on the subject.

The field which M. Pector has chosen for his studies is one rich in itself, and abounding in significance for the ancient ethnography of both American continents. In that narrow isthmus were centred and compressed the migratory streams from the north and south; and the problems of those migrations must look there for their solutions.

The Republic of Costa Rica lies at its southern extremity; and, concerning its ethnography, two recent works deserve prominent mention. The one of these is by Señor Manuel M. de Peralta, a pamphlet bearing the title, "Apuntes para un Libro sobre los Aborigenes de Costa Rica," Madrid, 1893. With a great deal of care and a singularly thorough knowledge of sources, the author has collected a surprising amount of material regarding the names, localities and affinities of the tribes who inhabited the region at the time it first became known to European observers.

Complementary to this, giving, on the other hand, the condition of the native tribes as they are to-day, is the Viaje de Exploracion al Valle del Rio Grande de Terraba, of Mr. H. Pittier, Director of the Physico-Geographical Institute of Costa Rica, (printed at San José de Costa Rica). The author is primarily a botanist and geologist, but his observations on the Terrabas, Bruncas and allied tribes are fresh, and full of information.

A MEETING of the Essex Institute, Salem, Mass., in memory of its late president, Henry Wheatland, will be held at Academy Hall, Salem, Monday, April 17, 1893, at eight o'clock P.M. Vice-President Goodell will preside, and addresses are expected from Honorable R. S. Rantoul, Professor E. S. Morse, Rev. E. C. Bolles, D.D., and others.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

GLACIATION IN AUSTRALIA.

BY T. S. HALL, CASTLEMAINE, VICTORIA, AUSTRALIA.

EVIDENCES of one or more glacial epochs are plainly visible in Australia, and the more closely is study directed to the subject, the more widely spread are the glacial deposits found to be. As long ago as 1861 Selwyn, then the Director of the Geological Survey of Victoria, noted that several conglomerate beds in various parts of the colony were evidently the results of ice-action, although no striated stones were visible. In 1877 Professor Tate of Adelaide announced the discovery of a glaciated surface near that city, and toward the close of 1889 Mr. E. J. Dunn found grooved stones in Victoria. Since then the Mining Department of Victoria has issued a report by Mr. Dunn of one of these conglomerate beds near Heathcote. The deposit covers about 36 square miles, and consists of a base composed of dark indurated clay, through which are scattered masses of rocks of various kinds—granites, syenites, gneisses, schists, quarzites, slates, shales, conglomerates, etc., etc. Many of the granites are not known in Victoria, *in situ*, and their origin can only be guessed at at present. In one or two places glaciated surfaces are seen and the striæ run north and south. The largest "erratic" known is a block of extra-Victorian granite, weighing about 30 tons. The thickness of the beds is estimated at about 400 feet. The bed rock is of Lower Silurian age, and is tilted at a high angle. Intercalated beds of sandstone occur in places, and show the deposit to be still nearly horizontal. In a paper recently read before the Royal Society of Victoria, Messrs. Officer and Balfour record grooved pebbles, "contorted till," and glaciated surfaces near Bacchus Marsh. The deposit there has, moreover, been heavily faulted.

The age of the Victorian deposits has not been precisely fixed as yet. At Bacchus Marsh the beds are overlain by fresh water sandstones containing *Gangamopteris, Schizoneura,* and *Zeugophylletes* (?), and which are stated by M'Coy to be of Triassic age. The age of the glacial beds is then perhaps Palæozoic. No fossil remains have as yet been found in the glacial beds themselves, but doubtless careful washing of the clays will yield evidences of life, as it has done in other countries. Small outliers of these beds are found widely scattered over the colony, from north to south, and on both sides of the Dividing Range. They extend into New South Wales, and may be looked for, Dunn says, at the foot of the western slopes of the Great Divide. Similar beds occur on the eastern edge of the great Queensland Downs

Mr. Dunn draws a parallel between these beds and the Dwyka conglomerates of South Africa, which are of Triassic age. If the parallel prove a good one, then we have evidence of an enormous extent of glaciation at the close of the Palæozoic or the beginning of the Mesozoic, extending nearer the equator than that of the Northern Hemisphere, during the last great ice age. The South Australian beds at Hallett's Cove, near Adelaide, before alluded to, are of Tertiary age. Here the glacier path can be traced for about two miles, and moraine *débris* is in abundance. Traces of more recent glacial action are recorded from the neighborhood of Mount Koscius Ko, but these are of local origin, and are perhaps due to a greater elevation of the region, as no glaciers exist in Australia at the present time.

SECRETS OF THE ATMOSPHERE.

BY H. A. HAZEN, WASHINGTON, D.C.

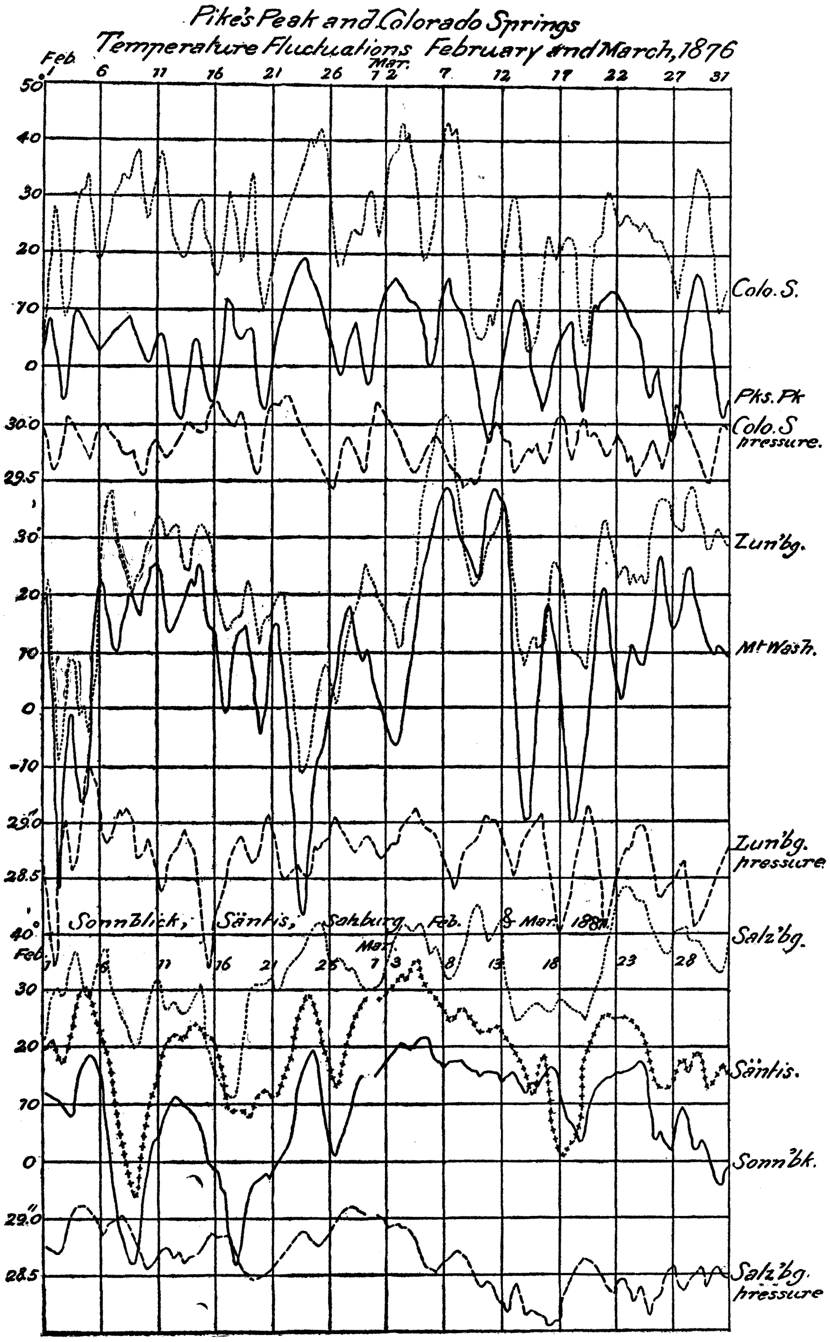
IN the March number of the *American Meteorological Journal*, Professor Harrington treats at some length the subject, "Exploration of the Free Air," and urges the great necessity of such an enterprise. For more than eight years the present writer has insisted that by no other means will it be possible to set the science of meteorology upon a firm basis and rid it of mere speculations and theories which too often have served to prevent its advance in the past. Professor Harrington quotes a graphic description of an experience of the aeronaut Wise, in which he seemed to be thrown or attracted back and forth in an ominous thunder-cloud. Several such have been described by aeronauts, who unfortunately had not the instruments requisite to give very necessary information in these cases and to make them of avail in a scientific study. The description of these mysteries make us long for something more tangible and definite.

To my mind there is no research of so great importance in the whole range of science as that of a few well conducted ascensions, with accurate instruments, in the midst of a rain-storm and on all sides of a low area. Ordinarily, balloon voyages have been made during clear weather and for the benefit of a great assemblage, so that this field, or the problem of ascertaining the secrets of the air, has been almost entirely neglected up to the present. A single illustration will show the extreme necessity of systematic work in this line.

It may not be generally understood that there has been an extraordinary revolution in meteorology within the past six years. During this revolution the whole convection hypothesis of storm generation, without the least doubt the most important of all the theories of orthodox meteorology, has been attacked and completely overthrown. The significance of this defeat cannot be exaggerated and should be fully set forth. The convection theory is fully advanced in Professor Ferrel's last book, published in 1890, "A Popular Treatise on the Winds," p. 228. "On account of the non-homogeneity of the earth's surface, comprising hills and valleys, land and water, and dry and marshy areas, all with different radiating and absorbing powers, and also on account of the frequently irregular and varying distribution of clouds, it must often happen that there are considerable local departures of temperature from that of the surrounding parts; and if it should so happen, as it frequently must, that this area is of a somewhat circular form, and the air has a temperature higher than that of the surrounding part of the atmosphere, then we have the conditions required to give rise to a vertical circulation, with an ascending current in the interior, as described above. But unless there is some source of heat by which this interior higher temperature is kept up, this circulation soon ceases, for the interchange of air between the interior and exterior parts of the air comprised in the circulation tends to continually reduce the difference of temperature upon which the circulation depends, and to bring all parts to the same temperature. . . . In the case of a moist atmosphere with the unstable state for dry air, we have the same energy for originating and maintaining a vertical circulation as in the case of dry air, with the additional energy of all the latent heat of the aqueous vapor set free in its condensation

in the ascending current, and this latter is a continuous source of energy as long as moist air is being drawn in from all sides to supply this current."

These are the careful words of one who devoted more than thirty years to a most thorough study of this whole question. I am well aware that certain disciples of Professor Ferrel are trying to take advanced ground on this vital question, and are striving to show that we may have a storm with lower temperature in its upper portion, but it seems to me this is a fatal error, and if persisted in must overthrow the convection hypothesis. I do not see how there can be any middle ground in this matter. If we accept Ferrel's views, we must stand by the convection hypothesis. It is well known that I have taught for many years that the convection hypothesis is disproved by the most convincing facts, and cannot possibly be sustained. It is none the less true, however, that the temperature in our storms up to great heights is vastly higher than that of the surrounding air, and in our high areas it is vastly less. These facts are absolutely established by observations on mountain-tops. I give here curves showing the fluctuation of temperature at the base and summit of the following stations: Pike's Peak (14,134 feet, highest meteorological station in the world), and Colorado Springs (5,950 feet); Mount Washington (6,279 feet), and Lunenburg (1,100 feet); also at Sonnblick (10,170 feet), and Salzburg (1,434 feet), in Austria. In the last set of curves I have added Säntis (8,202 feet), situated 165 miles west of Sonnblick. Any one studying these curves must be convinced that the temperature fluctuations are precisely the same at the summit and base of each high station. Now it is a universal law that on the approach of a storm the temperature rises at sea-level, and with a high area there is a decided fall, especially in the colder months. This law is abundantly borne out in these curves, for a comparison between the pressure curve (broken) at the base and the temperature curve (dotted) shows opposite phases between the two, and the temperature maxima and minima occur earlier at the summit than the pressure minima and maxima at the base. This is shown most clearly at the stations in the United States, but it can be seen also at the Austrian stations.



To my mind, it is impossible to conceive of a normal storm, seeming to move at the rate of 30 or 40 miles per hour along the earth, which does not have a higher temperature up to a height of at least 5 miles, and probably much higher. The reverse of this must also be true in the case of a high area. In fact, I utterly fail to see how an area of high pressure can have a rapid motion unless in its centre there is denser air, brought about by a greater degree of cold, and this, as I understand it, is exactly the view of Professor Ferrel. These views were generally held up to 1886, and I am not aware that any one disputed them, except as regards the pure convection hypothesis.

In 1886 M. Dechevrens wrote a paper in which he tried to show that the usual law of the relation of pressure and temperature at the earth's surface was exactly reversed at the height of Mount Washington (6,279 feet), and that at that point a fall in temperature occurred with a fall in pressure, and *vice versa*. This was a most astonishing result, and seemed to disprove the whole convection hypothesis. His research consisted in studying the pressure and temperature fluctuations at the summit without any reference to the passage of storms or high areas at the base.

In this study he ignored the fact that after the passage of a storm the cold wave following would tend to contract the air below the summit, and hence the pressure would continue to fall, and the minimum would not be reached till some time after the passage of the storm at the base. Exactly the reverse conditions would be found on the passage of a high area. A full analysis of this proposition, with curves showing these effects, will be found in Annual Rept., Chief Signal Officer, 1882, pp. 897-902.

In addition to this difficulty in comparing these fluctuations directly, there is another almost as serious, which lies in the fact that the maximum and minimum points in the temperature oscillations occur several hours earlier at the summit than they do at the base. It will be seen readily that both these conditions would tend to bring the minimum of temperature at the same time as the minimum of pressure at the summit. How closely these conditions of temperature at the base agree with those at the summit we have already seen perfectly demonstrated in the curves given above. This paper of Dechevrens was translated and published with comments dissenting strongly from the views advanced in the *American Meteorological Journal*, August, 1886, pp. 297-314.

It is probable that these researches would have attracted very little attention had it not been for a study by Dr. Hann of observations at Sonnblick (10,170 feet) on almost exactly the same lines as those pursued by Dechevrens, and with the same result. The first paper by Dr. Hann was published in April, 1887, in the *Meteorologische Zeitschrift*, and this was followed by others in the same and other journals, the last and most thorough research of all, of 86 pages, appearing in April, 1891. In this study the conditions were still farther complicated, from the fact that Sonnblick lay in southern Europe, where very few, if any, normal storms or high areas pass. High areas, with a pressure of 30.7″, have been known to hover over this region for three weeks at a time. Such conditions are unheard of in the United States, and their effect can be at once recognized by comparing the fluctuations in the lowest series of curves with those in the other two in the diagram already given. It is easy to see that under such conditions the stagnant air above would become abnormally and cumulatively heated day by day, while the lower air in a clear sky would be abnormally cooled, and there would at times appear to be a reversal of temperature. With all these disadvantages, however, it will be seen that the temperature curve at the base of Sonnblick, in its larger oscillations, agrees almost exactly with that at the summit.

One of the most remarkable results found by Dr. Hann was that the maximum and minimum points of temperature lag a day behind those of the maximum and minimum air-pressure. It seems almost incredible that such a deduction could have been made. It seems as if it could only have been by confusing or comparing the minimum air-pressure oscillatian of a storm with the minimum temperature oscillation of the following high area. An examination of the diagram already given brings out this fact most clearly. In nearly every case, both at Pike's Peak and Mount Washington, the minimum of air-pressure occurs a day earlier than the minimum of temperature accompanying the succeeding high area. I am sure that no one will make this mistake on studying the diagram. The maximum of temperature accompanying a storm occurs about a day earlier than the minimum of pressure accompanying the same storm. The minimum of temperature accompanying the succeeding high area has nothing whatever to do with the previous minimum of pressure, and a proper study of the diagram shows at once the truth.

It seems to me my position in this matter is brought out most clearly and distinctly in the last paper by Dr. Hann, of 86 pages, in April, 1891. Speaking of fluctuations of pressure and temperature at p. 367, Dr. Hann says: "Für die Erdoberfläche sind dieselben seit Langem bekannt." "For the earth's surface have these been for a long time well known." I am sure that every one will admit that at the earth's surface as a storm comes up the temperature rises and is the highest during the storm. As a high area advances, the temperature falls, and is generally vastly lower during such pressures than during storms, especially in the winter season. On page 370 Dr. Hann gives the temperature conditions during barometer maxima, and on page 375 the conditions with minima. The sea level station at the base of Sonnblick was Ischl, and I give here the temperature in both maxima and minima during the colder months.

Temperature at Ischl, Cold Months.

	Barometer Reading.		
	Maximum.	Minimum.	Difference.
	Centigrade.	Centigrade.	Centigrade.
October	8.4°	6.5°	1.9°
November	2 8	2.4	0.4
December	− 2.6	− 3.3	0.7
January	− 1.5	− 0.3	− 1.2
February	1.0	− 4.8	5.8
March	4.0	− 5 1	9 1
Mean	2 0	− 0.8	2 8

This table shows that at the base of Sonnblick during every month except one the temperature is higher in a maximum barometer reading than in a minimum, and the average difference is 2.8° C., or 5.0° F. This exact and marked reversal of the universal law is very significant and proves conclusively that there has been either a most serious error in studying or selecting out the different cases, or that the universal law does not hold for this region.

It would seem that Dr. Hann himself now recognizes the difficulty in using these records, for he says, in a paper on Ben Nevis (4,406 feet), *Meteorologische Zeitschrift*, December, 1892, p. 457: "Wie Herr Buchan schon in seinem ersten Bericht hervorhebt, zeigt auch der Ben Nevis sehr häufig die Erscheinung hoher Temperatur und grosser Trockenheit selbst mitten im Winter, sobald er in das Gebiet eines Barometermaximums zu liegen kommt. Es treten dann auch öfter die sogenannten Temperaturumkehrungen auf, was hier besonders bemerkenswerth ist wegen der freien Lage von Fort William am Meere, welche eine Stagnation kalter Luft an der unteren Station ausschliesst." "As Mr. Buchan showed in his first report, very often Ben Nevis has the appearance of higher temperature and greater dryness in winter as soon as a barometer maximum lies in that region. It shows then often the so-called temperature reversal, which here is especially remarkable because of the free position of Fort William on the sea, which prevents a stagnation of cold air at the lower station." The question seems to be clearly set forth in these words, and it is probable that the advocates of the view that in the centre of our high areas there is a rise in temperature at some height above the earth will be willing to stand or fall by the proofs at Ben Nevis.

The date of this abnormal heat was Dec. 31, 1883.[1] Another occasion was on Nov. 18, 1885, and a third on Feb. 5, 1886. These are the only marked cases from December, 1883, to February, 1886, though there were minor cases of no importance on Jan. 16 and Dec. 22, 1884, and on Nov. 10, 1885. I have made a careful search of all the published observations for maximum barometer readings in the four cold months, and have found 70 cases. That is to say, out of 70 cases, only 3 show a marked departure from the law that there is the same oscillation of temperature at the summit as at the base of Ben Nevis. But this is not all. On Dec. 31, 1883, the motion of the high area was quite slow and the wind on Ben Nevis almost a calm, thus causing a stagnant air. On Dec. 28, or three days earlier, the temperature at the summit began falling, and in 24 hours it had fallen nearly 22° F., or *more* than at the base in the same time. This shows that the usual law was acting even in this case and that the subsequent rise was due to an abnormal condition and not to the fact that the temperature was higher in the centre of the high area than on either side.

In the other case cited by Dr. Hann on Nov. 18, 1885, the conditions were exceedingly abnormal, as the high area moved from the *east* toward the *west*. It would be impossible to reason as to the ordinary temperature conditions in a high area from such a case. It is an interesting fact that in the latter case the oscillation of temperature at the summit was precisely the same as at the base, except that the fall and rise at the summit was a little greater than at the base, and it took place about 24 hours later, instead of earlier as is usually the case. The usual law of lower temperature in the centre of a high area is abundantly borne out at Ben Nevis, and I have found the reverse law in the centre of a low area also true at that station.

I have thus dwelt at some length upon these studies for the reason that they have been largely accepted by European meteorologists and have served to overthrow nearly every hypothesis that has been regarded invulnerable in the past. Is there not here the best proof in the world of the extreme need of an exploration of the atmosphere at the seat of these disturbances? Meteorology needs, above all things else just at present, a full and complete setting forth of the facts to be gleaned in the upper atmosphere. An array and study of these facts would give us a good foundation on which to lay the corner-stone of a good and exact science. It would be of inestimable value in forecasting the weather and in removing our ignorance, which is so serious a drawback at present.

We do not know positively the simplest conditions in the atmosphere. Glaisher once left London in a pouring rain and emerged into clear sky after rising only 800 feet. At another time he found rain falling in a cloud 15,000 feet high. In this country no rain has been observed in balloon ascensions above 9,000 feet, and it is probable that a large part of our rain forms at a height less than 6,000 feet. We do not know the thickness of a rain-cloud nor its temperature. Some think the temperature must be higher than that of the surrounding air, else the storm would quickly cease; others think that no rain can form unless the temperature is lower than the outside air. Our books are full of speculations as to the dynamic heating of the air and the conditions needed to originate and maintain our storms and high areas. The evidence seems quite clear that all these theories, often contradictory among themselves, would not account for a tithe of the energy displayed, and an exploration is needed to determine this fact, or to establish the truth.

Is there an ascending current in our storms, or a descending one in our high areas? These are theories of the deepest interest. The evidence seems to show that there is not a transfer of an air-mass in any direction, either up or down or horizontally, in our storms or high areas. Professor R. H. Scott, after giving all possible sources of rain formation, decides that the only one that can be maintained on theoretical grounds is that rain is formed in an ascending current of warm, moist air. A determination of this question would be of inestimable value in all studies and researches as to the natural or artificial formation of rain.

[1] Misprinted 1884.

In several ascensions in this country it has been found that there seem to be rather definite layers of moisture even in a clear sky. Sometimes two layers have been found at different heights. These would seem to be exceedingly significant facts. Do these layers serve as conductors for electric currents, as seems to have been very guardedly stated by Professor Loomis? How do these layers thicken as a storm comes up, or, rather, is the thickening process a precursor to the storm? Does this thickening in a certain definite direction show in what direction the storm will subsequently move, or is it caused by the conditions accompanying the storm? Do these layers rise or fall, or what is their movement under different atmospheric conditions?

What relation does the dust in the atmosphere bear to these layers? Is there an increase of dust in definite layers? Is dust needed to produce this thickening? It seems to me the careful and painstaking investigations of Professor Barus in cloud condensation must bear valuable fruit as soon as he turns to the ordinary conditions in our storms, and for this the study can be prosecuted only with great difficulty, except in nature's own great laboratory.

A serious drawback in the past to successful balloon exploration has lain in the lack of suitable instruments. Professor Glaisher often took up instruments enough to stock a meteorologic observatory, and in a single ascension once broke nearly $500 worth. What is needed is an instrument that can be read very quickly, once a minute if possible, and, at the same time, do its work very accurately. A sensitive aneroid will give the pressure, and a sling psychrometer will give the moisture conditions. Various rather singular objections have been raised to this instrument. One is that it will give 5° too high temperature under strong insolation. This experiment has been tried, and it is known that under the strongest insolation possible the temperature will be less than .8° higher in the sun than in the shade. Another objection raised has been that it will give a lower relative humidity in bright sunshine than in shade. This is entirely wrong, because the muslin coating of the wet bulb is a vastly better absorber of heat than the bright bulb, and hence, if anything, in bright sunshine the relative humidity must be higher than in shade. It is also said that the heat of the balloon will tend to raise the temperature of the sling thermometer because it cannot be used far enough away from the basket. In a comparison between the sling thermometer and another so-called standard (aspiration thermometer) the greatest difference between the two occurred when the balloon was moving horizontally, and the least when the balloon was ascending most rapidly, so that this objection utterly fails. The true criterion of an accurate instrument is that it shall give the same temperature of any stratum in a rapid ascent and descent, and this is fulfilled in a marked degree by the sling psychrometer. I have used this instrument for over eight years and in five balloon voyages, and am satisfied that it is a perfect instrument and one that responds at once to any demands put upon it.

The expense of ballooning in the past has been enormous, and a serious drawback to its prosecution. One is amazed to read that in certain high ascensions, to five miles and over, the balloon of 90,000 cubic feet capacity was filled plump full, thus necessitating the carriage of about a ton and a half of ballast. This ballast had to be poured out and more than half the gas wasted before reaching the height desired. It is no wonder that the aeronaut was completely exhausted with his labors with the ton of ballast. All this gas that had to flow out, because of expansion, was a dead loss, say, $150 for each ascension, and after landing the remaining gas was emptied. All of this expense can be avoided, as I am firmly convinced. It is well known that if a balloon leaves the earth at all, it will rise till the envelope is plump full. If the balloon will rise when two-fifths full of gas, it will continue to do so till it has reached more than five miles, the limit desired at present, though there is no reason why ultimately we may not ascend to the extreme limit to which hydrogen may carry us by the use of a pneumatic cabinet. It is proposed to employ a small balloon with hydrogen gas. A balloon of 20,000 or 30,000 cubic feet will easily carry two men when half full, and the enormously less labor of handling it, as com-

pared with that of handling one of 100,000 cubic feet, can hardly be estimated. The risk in a high wind of the smaller balloon is vastly less than of the larger. Every way the smaller balloon presents advantages over the larger. The first cost of a balloon of 20,000 cubic feet would be $600. The cost of a half charge of gas need not be over $30, and may be less. It is hoped that the balloon will be sufficiently tight to hold its gas for a long period. In Europe balloons have been made with gold-beaters' skin that have leaked only ½ of 1 per cent in 24 hours. I think the leakage of a cloth balloon when properly made need not be over 4 or 5 per cent, but the figures in this country are exceedingly meagre and unsatisfactory. After an ascension it will be a very simple matter to conserve the gas, and, if wished, an addition may be made at the landing-point with gas from a flexible holder, which may be easily transported from point to point.

An interesting problem presents itself as to the behavior of the gas in a rapid ascent or descent. Theory indicates that in a rapid expansion dry gas will cool 1° F. in 186 feet ascent, so that at 25,000 feet the temperature would be about 130° lower at the centre of the balloon than at the outside air, provided the ascension was quick enough to prevent the heat from striking in. Now experience in balloon ascents shows that the gas in a balloon is invariably *warmer* than the outside air. Exactly the reverse is true in a rapid descent, both as regards theory and practice. Whether this is due to the fact that the envelope retains its heat or not, it still remains that we have here apparently a means of making our ascensions with the loss of little or no gas at the valve. At the highest point our gas will be cooled and lose its buoyancy, which allows a fall in the balloon, which is always greatly accelerated as we approach the earth, and after landing the balloon may be anchored till the sun's heat has warmed the gas, which will enable another trip with the same gas.

The risk in such ascents has been greatly exaggerated by some from the serious and often fatal accidents that have attended jumping with parachutes and ascending in hot-air balloons. The modern balloon, with its very long drag-rope and rip-cord, are very safe. Even in case the balloon should burst, the envelope catches in the netting and acts like a parachute in breaking the fall. Mr. Wise, the veteran, once ascended to the height of a mile and purposely exploded his balloon in order to show that there was no great risk in such an adventure. In one case, Mr. King and a married couple were in a balloon which exploded at the height of a mile, and without serious consequences It should be noted that a new balloon will not explode. Glaisher reports having ascended with a balloon full of gas at the rate of 4,000 feet per minute; this was a remarkable feat. It is not the intention to ascend faster than 1,000 feet per minute, and at this rate the danger of bursting is almost nothing.

Some may think that such observations may be made at vastly less risk, expense, and discomfort on mountain tops. Undoubtedly there are some observations of temperature that may be made in this way, but even in this case we cannot tell just what effect the summit will have. Observations of rainfall, clouds, electricity, etc., are entirely impossible on mountain-tops, for the reason that these have a peculiar action of their own entirely different from that of the free air. It seems probable that the mountain acts like a point in the atmosphere from which there is a continuous discharge of electricity, as in the case of a point on the conductor of an electric machine.

The exploration of the atmosphere cannot be carried on in Europe to as good advantage as in this country, for the reason that they do not have the normal low areas and high areas travelling at some velocity that we have. The conditions of the atmosphere are so different in the two countries that we must make our own researches. I trust I have shown the great need of such exploration. I know of no endowment of $5,000 or $10,000 that would pay so rich and immediate a harvest as this for ballooning. Thousands are spent in visiting the inhospitable north, while a field just at our hands, which may be explored at vastly less expense and risk, and which promises immeasurably greater returns, is left unexplored and unvisited.

March 31, 1893.

LOSS OF DRY MATTER BY THE SPROUTING OF CORN-SEEDS.

BY E. H. FARRINGTON, CHEMIST, AGRICULTURAL EXPERIMENT STATION, CHAMPAIGN, ILL.

SEEDS of the corn-plant were placed in damp cotton and left to sprout in the dark for nine days. Four of these seeds partially sprouted, then moulded, failing to develop further. They lost by this treatment 9 to 18 per cent of the dry matter in the original seed.

Two seeds, under the same conditions for nine days, sprouted and developed a corn plant. The root and stem of these plants each measured two to three inches, and their weight was from three to three and one-half times that of the original seed. It was found, however, that when the water was dried out of these young plants the dry matter in them was 20 and 31 per cent less than the seed contained.

Several estimations were made of the per cent of water and dry matter in a sample of corn. These results were used for estimating the weight of water and dry matter in the corn which was taken from the same sample and sprouted.

Details of Weights in Grams.

	Dry Matter.	Water.	Total.
Weight of seed before sprouting	0 271	0.042	0.313
After nine days sprouting in damp cotton, plant with seed attached, weight	0.187	0.747	0.934
Gain or loss of plant over seed	− 0.084	+ 0.705	+ 0.621
Per cent gain or loss was of weight in the seed	− 31.0	+ 1680	+ 198
Duplicate observation gave	− 19.8	+ 1945	+ 239

This shows that in sprouting the white plant had taken up water but lost in dry matter.

This experiment was repeated June 3, 1892, by sprouting the seed in the soil of a corn-field instead of cotton. One week after planting, four of the plants were dug up. They were about two inches above ground and had two green leaves. The shell of the seed still clung to the plant. The root was about five inches long, making a total length of about ten inches from tip of leaf to end of root.

The weight of these green plants, free from soil, was about four times that of the seed planted, but they contained from 58 to 79 per cent only of the dry matter in the original seed.

During the week these seeds were growing the climatic and soil conditions were ideal for corn.

Details of Weights and Measurements.

Plant No.	Weight in Grams.			Measurement of Plant. Inches.			Dry Matter.	
	Seed.		Green Plant.	Tip of Leaf to Seed.	Above Ground.	Roots.	In Plant.	Per cent of that in Seed.
	Dry Matter.	Total.						
1	0.416	0.479	1.638	4	2	5	0.331	79.3
2	0.357	0.412	1.447	4½	2	5	0.210	58.8
3	0.347	0.450	1.549	3½	2	4	0.273	78.6
4	0.393	0.457	1.454	1½	3½	4	0.310	78.2

Two weeks after the seed had been planted, five plants were cut at the surface of the soil, and the weight and measurements of each plant above ground was compared with the weight of the seed. This shows that corn-plants, having a height of ten to fourteen inches above ground, weighed when green four to eight

times as much as the seed, but the dry matter in these plants was from 86 to 130 per cent only of that in the seed planted.

Details of Weights and Measurements.

Plant No.	Weight in Grams. Seed. Dry Matter.	Weight in Grams. Seed. Total.	Weight in Grams. Green Plant.	Height of Plant above Ground. Inches.	Dry Matter. In Plant.	Dry Matter. Per cent of that in Seed.
1	0.378	0 437	3 235	14	0 493	130.4
2	0.346	0.400	1.79)	9½	0.3 0	86.7
3	0.395	0.456	2.470	11½	0.435	110 1
4	0.404	0.466	2.610	11	0.348	86.1
5	0.424	0.490	3.540	12	0.437	103.0
Growth above ground of two plants three weeks after planting.						
1	0.348	0.402	16.60	21½	1.826	524.6
2	0.413	0.477	18.60	20½	2.045	495 4

ELECTRICAL NOTES.

Some of the practical results of Dr. Sumpner's work on photometry were alluded to in a previous note. As the Proceedings of the Physical Society are not generally accessible, and most of the abstracts given are rather brief, it may be worth while to give a short account of some of the more theoretical results.

The first is the practical demonstration of the very approximate accuracy of the cosine law of reflection of such substances as white paper, tracing cloth, and white cloth. From this follows the remarkable result, confirmed by experiment, that placing a piece of white paper behind a source of illumination more than doubles the illumination at a point normal to the plane of the paper, while the placing of a mirror in the same position does not quite double it. The reason of this is at once seen to be the fact that the reflecting power of white paper and the mirror are about the same, but that, of a given amount of light falling on the paper, in consequence of the cosine law, the greater part is reflected normally to its surface, whereas in the case of the mirror, the absorption of the glass is greatest in the case of the light falling perpendicularly to it, and so the greater part of the light is given off in directions which are not normal to the surface.

In the discussion following, it was pointed out that no known shape of the roughnesses would lead to the mathematical deduction of the cosine law, so it is probable that the phenomenon of diffusion of light is of a somewhat more complicated nature than is generally supposed. It is to be hoped that the definitions used by Dr. Sumpner will be generally employed in photometric work. They are as follows:

1. Candle-power.— The candle-power of a lamp is measured by the ratio of the illumination of the light considered, to that of a standard candle, both sources being at the same distance from the object illuminated.

2. Illumination.— The unit of intensity of illumination is that given by a standard candle at a distance of one foot.

3. Unit quantity of light.— Unit quantity of light is the quantity of light which falls on a surface of one square foot placed at a distance of one foot from a standard candle, and so that a normal drawn to the surface at any point, passes through the source of light.

The name candle-foot is given to the unit quantity of light.

From the definition, a source of light, candle-power X, gives out a total quantity of light equal to 4 π candle-foots.

4. Brightness.— This definition only applies to solids which become sources of illumination, either through incandescence, as heated platinum, or through reflection, as paper exposed to sunlight, i.e., only to such substances as obey the cosine law.

A surface has unit brightness when a point at a distance of one foot from a surface of one square foot of the substance, and so placed that a normal drawn from any point of the surface passes through, the point, is illuminated with unit intensity.

From the definition, it follows that the total quantity of light given off by one square foot of surface of brightness, X is πX.

One interesting result, following from the considerations which lead to the last of these definitions, is that given by Dr. Sumpner, as it affords an explanation of snow-blindness.

The total quantity of light reflected from the snow will nearly equal the amount which falls on it. Therefore, if C be the intensity of the illumination of the sun at the surface of the snow, the brightness of the snow at a distance of one foot from it will be C|π. Therefore, if the observer is standing so that the snow-field subtends a solid angle of 90 degrees, we may easily find that the illumination at the point where his eye is, is nearly C, or that the effect is nearly the same as if he were looking straight at the sun. R. A. F.

LETTERS TO THE EDITOR.

*** *Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Early Attempts at Storm-Warning.

IN reading Haweis' "Music and Morals," I found on page 368 a statement of interest to meteorologists. Writing of the famous Strassbourg tower, he says: "The second bell, recast in 1774, is named 'The Recall' or 'Storm-Bell.' In past times, when the plain of Alsatia was covered with forests and marsh land, this bell was intended to warn the traveller of the approaching storm-cloud as it was seen driving from the Vosges Mountains towards the plain."

Probably Kopp, Günther, van Bebber, or Hellmann, in their records of antiquarian research, have mentioned this early attempt at storm-warnings, but I do not remember having seen anything about it. FRANK WALDO.

Princeton, N.J., April 5.

Pre-Historic Remains in America.

IN his letter in *Science*, March, 31, under the above title, Professor Cyrus Thomas misunderstands the quotation which he makes from my "American Race." He observes, "If the settlement was at one point by one race, and this race was never influenced by another, it is difficult to imagine in what respect the moulding process acted." Is it? Plainly the moulding process acted by modifying the intrusive population to another and a fixed racial type by long subjection to an environment to which previously it had never been exposed. Nothing is better recognized than such a process; it is taken for granted by all writers, as, for instance, by Dr. Braislin in the same number of *Science* in which Prof. Thomas's letter appears; and why such an objection should be offered to my statement, it is even more "difficult to imagine."

The general theory advanced by Professor Thomas of a fundamental difference between the civilizations of the Atlantic and Pacific groups, is one for which I have never found any evidence. He must know that the ancient civilization of the Mississippi Valley offers as strong, if not stronger, traits of analogy to that of Mexico and Yucatan than does that of the Haidahs. Consider the designs shown on the engraved shells, so well shown in the beautiful monograph of Holmes, or the copper work of the mounds of Ohio and Georgia! In view of such evidence, how could Prof. Thomas write, that "no such resemblance to those of the Atlantic slope is observable?" Is he not also aware that both the Nahuatl and Maya languages trace their affinities exclusively to the eastern and not to the western water-shed of the continent

As for the "mathematical probability" referred to by Dr. Dall, it is illusory. We find "interwoven chains of customs and belief" of the most seemingly fanciful and artificial character in nations so remote that the theory of transmission is impossible —such as Niblack shows between New Zealanders and Haidahs, or as Morgan adduced between Iroquoiean and Dravidean tribes. These do not depend on transmission, nor yet on chance, but on the unalterable principles of human psychical development, which proceeds under fixed laws, operates largely on the same or similar materials, and produces identical or analogous results.

In conclusion, I repeat what I have said more than once before, that I challenge any one to cite a single American language showing clear traces of Asiatic or any other foreign influence; or a single native American art or industry obviously traceable to foreign culture. D. G. BRINTON.

Philadelphia, April 5.

Auroras.

SINCE 1572 there have been 106 auroras seen as far south as the Mediterranean in Europe or Virginia in this country, and exhibiting features constituting displays of the first magnitude. In making up this list, the records consulted have been sufficiently complete to insure that very few, if any, displays, having the geographical extent indicated, have been omitted. The list comprises, practically, all the really great auroras during the past 420 years, few, if any, of which would have failed to be visible even in full moonlight or strong twilight. It is a very curious fact, that very few of these splendid displays reported from large numbers of localities and attracting the attention of even the most indifferent, fall near the solstices, while they are most numerous near the equinoxes. This peculiarity has long been known, but that the distribution is real and not factitious, depending upon twilight in the summer and cloudiness in the winter, is best shown by admitting only those auroras which are certainly on a sufficiently grand scale to insure that they will without fail be seen and widely reported. The monthly distribution of the displays belonging assuredly to this class during the past 420 years, is as follows:

January	6
February	17
March	14
April	8
May	3
June	0
July	4
August	4
September	14
October	21
November	12
December	3
Total	106

M. A. VEEDER.

Lyons, N.Y.

The Palæolithic Man Once More.

IN the first number of the new *Journal of Geology*, published under the auspices of the University of Chicago, Mr. W. H. Holmes, in the capacity of co-editor in "Archeologic Geology," has given to the world a long and labored article, in which he endeavors to demonstrate that because he has failed to find any evidence of the existence of the palæolithic man in the Trenton gravels, therefore no such evidence has ever been found by any one else. In his characteristic style he designates as "gravel searchers, unacquainted with the nature of the object collected and discovered, and little skilled in the observation of the phenomena by means of which all questions of age must be determined," several of the foremost men of science of our time, who claim to have discovered such evidence there. As he also makes

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Apr. 11.—Frank Hamilton Cushing, Zuni Song and Dance.

Biological Society, Washington.

Apr. 8.—J. W. Chickering, The Botanical Landscape; Frederick V. Coville, Characteristics and Adaptations of a Desert Flora; C. W. Stiles, Notes on Parasites,—the Cause of "Measly Duck," with Microscopic Demonstration; R. R. Gurley, Natural Selection as Exemplified by the Cackling of Hens.

Geological Society, Washington.

Apr. 12.—Symposium—Subject: The Age of the Earth, taking as a basis for discussion the article by Mr. Clarence King in the *American Journal of Science* for January, 1893. The discussion was opened by Mr. Gilbert, and many others participated.

the astonishing statement in the same article that "most of these so-called gravel implements of Europe are doubtless the rejects of manufacture," pre-historic archæologists, and especially those of Europe, will draw their own inferences as to the qualifications for pronouncing an *ex cathedra* decision of the man who made the statement (*Science*, Nov. 25, 1892) that "there is not in the museums of Europe or America a single piece of flaked stone found in place in the gravels of America and satisfactorily verified that can with absolute safety be classified as an implement at all."

I have had occasion elsewhere to refer to Mr. Holmes's fondness for making startling assertions, instancing his statement about the Indians, in the same article in *Science*, that their "quarrying was accomplished mainly by the aid of stone, *wood and bone* utensils, aided in some cases perhaps by fire. With these simple means the solid beds of rock were penetrated to depths often reaching twenty-five feet."

The readers of *Science* have lately had an opportunity of observing also that Mr. Holmes "strongly deprecates personalities in scientific discussion." HENRY W. HAYNES.

Boston, March 30.

BOOK-REVIEWS.

The History and Theory of Money. By SIDNEY SHERWOOD. Philadelphia, J. B. Lippincott Co. $2.

THIS book contains twelve lectures delivered in the university extension course at Philadelphia last year before a company of bankers and others interested in the subject of finance Half the lectures are professedly historical and the other half theoretical; but the historical element is really predominant throughout them all. This seems to us a mistake, for the history of money cannot be properly understood nor appreciated without a previous acquaintance with the theory, and in these lectures the theory is not stated with anything like the clearness and fulness which the subject demands. However, it was expected that the attendants on the lectures would read and study for themselves during the progress of the course, the books recommended for their use being named in this volume; and such reading would supplement the instruction given in the lectures. Mr. Sherwood, who is attached to the Wharton School of Finance in the University of Pennsylvania, shows a thorough familiarity with his subject, and, what is quite as important, he has no hobbies to ride, and is not prone to extreme or one-sided views. He begins by showing what money is for, what purposes it fulfils in the world's economy, and then proceeds to treat of the different kinds of money in use, with remarks on coinage, on the history of the precious metals, and on government notes and bank notes, with brief discussions of some of the many economic questions which those subjects involve. The lectures are expressed in a plain and straightforward style, which the hearers could readily understand, and they were evidently enjoyed by those who listened to them. For our part, however, we have found the discussions at the end of each lecture, and which are here reported in brief, the most interesting part of the book as well as the most suggestive. There were many persons in the audience well equipped with both theoretical and practical knowledge of the subject, and their discussions with Mr. Sherwood and with one another called up many points that were not touched upon in the lectures, and presented various and sometimes conflicting views. Among the debaters was a lady of socialistic proclivities, whose remarks and questions added variety and piquancy to the scene, though she did not appear to have many sympathizers. On the whole, though it cannot be regarded as an adequate scientific treatise on money, this book will certainly have an interest for all who care for its subject.

AMONG THE PUBLISHERS.

J. B. LIPPINCOTT COMPANY announce as in preparation "Vagaries of Sanitary Science," by F. L. Dibble, M.D.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale.—An Abbe binocular eye-piece for the microscope. Alfred C. Stokes, 527 Monmouth St., Trenton, N. J.

For sale or exchange.—One good long range Remington B. L. rifle, 44 calibre, also land and fresh water, and marine shells. Want shells, Safety, camera or printing press. A. H. Boies, Hudson, Mich.

Fine collection of microscopic slides for sale, or would exchange for first-class pneumatic bicycle. J. E. Whitney, Box 549, Rochester, N. Y.

For sale—A Zentmayer new model U. S. Army Hospital monocular stand. Price $110, will sell for $75. Address H. C. Wells, No. 151 Broadway, New York.

For sale—A complete set of the third series of the American Journal of Science (1870-1893) handsomely bound in single volumes in dark brown half morocco. Address G. H. Williams, 803 Cathedral Street, Baltimore, Md.

For sale, or for exchange for books on medicine or surgery, new editions only, a large geological library, containing nearly all the State and Government Reports since 1855. Will be pleased to answer letters of inquiry and give information. Address R. ELLSWORTH CALL, Louisville, Ky.

For exchange.—Slides of Indian Territory Loup Fork Tertiary Diatoms for other microscopic fossils. Address S. W. WILLISTON, Univ. of Kansas, Lawrence, Kans.

For exchange.—Will exchange an "Ideal" Microscope of R. and J. Beck, London, 2 eye pieces 3 objectives, 3 inch, 1 inch, 1-6 inch; bull's eye condenser on stand, substage condenser, mechanica stage, etc., for any of the leading makes of type writers. Particulars by mail. DELOS FALL, Albion College, Albion, Mich.

For sale or exchange. — I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

Wants.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

WANTED—Second-hand copy Tryon's Systematic Conchology. Please quote condition, date, price. H. L. Osborn, Hamline University, St. Paul, Minn.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R.Ellsworth Call,Louisville,Ky.

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